local as it appears when set forth without qualification. The objects which our Transatlantic colleagues are desirous of carrying out in the name of our distinguished countryman will, I am sure, have the entire sympathy of all English chemists, and, for my own part, I can only wish that every success may attend their celebrations next month. At the same time, I may point out that research in this country is very poorly endowed as compared with the munificent foundations for this purpose established by wealthy Americans. We may, therefore, while wishing that the American jubilee will be productive of all the results which they themselves desire, appeal with confidence to Americans to support also that other part of their own scheme which provides for the endowment of our Perkin research fund. R. MELDOLA.

Blair Atholl, Perthshire, September 15.

Horizontal Pendulums and Earthquake Echoes.

I HAVE just finished reading Captain Dutton's book 'Earthquakes in the Light of the New Seismology," the preface of which was written in April, 1904, while the date of publication is 1905. I mention these dates because a paper of mine, bearing particularly on chapter vii. of the book, was read before the Physical Society on March 13, 1903, and appeared subsequently in the Proc. Phys. Soc., vol. xviii.; it also was published in the *Phil. Mag.* for October, 1903. I gather, however, that Captain Dutton has not seen the paper, and as I am not certain of his exact address I am addressing you.

To be candid, I say that Prof. Milne has not considered the effect of resonance sufficiently, if at all, in computing the tiltings represented by his seismograms. Certain screws are placed in the base-plate of a Milne seismograph partly for levelling purposes and partly to give the base a known tilt. Prof. Milne has argued thus (*vide* almost any British Association report): if a tilt of 1″ given to the base displaces the end of the boom 1 mm., then a seismogram of 10 mm. amplitude indicates a tilt of 10″, if it indicates a tilt at all. On this assumption Prof. Milne discusses the tilting theory and discards it (the theory), and also on this view Captain Dutton's seventh chapter is based. That it is a wrong view my diagrams as well as ordinary mathematics show.

I have also given a more prosaic explanation of Prof. Milne's "earthquake echoes" than is to be found in either the British Association reports or in Captain Dutton's book, pp. 235–6. The tilt represented by any seismogram is a function of the boom period, the wave period, the log. decrements of the free vibration of the boom, and of the earth wave, and is, in general, very much smaller than that which is given in the British Association reports. Earthquake echoes may be regarded as interference effects between the free and forced vibrations. I notice that in the diagrams in my paper referred to the fifteen-second wave diagram which shows these interference effects best has been turned round with regard to the others. With the exception of this one, the artificial vibrations all start to the right, and are continued regularly until the end of the diagram is reached, when obviously the swing of the boom dies away naturally.

C. COLERIDGE FARR.

Canterbury College, Christchurch, New Zealand, July 21.

IN a report to the British Association in 1899 I directed attention to the fact that as an earthquake dies its seismograms indicate that it does so in a series of more or less rhythmically decreasing impulses. These, I suggested, were more likely to result from reflection than from interference, and therefore they were provisionally called echoes. Four years later (*Phil. Mag.*, October, 1903) Dr. C. Coleridge Farr discussed the terminal wave group as interference effects between the free period of the recording boom and the period of the ground. Theoretical considerations show their existence, whilst an ingenious experiment carried out by Dr. Farr shows that it is not difficult to reproduce wave groups strikingly similar to those shown in many seismograms. On the two sides of a pillar carrying a pendulum with a period of 16·5 seconds Dr. Farr attached two boxes filled with sawdust. Two chains connected by a rope passing over pulleys hung over the centre of each box. This arrangement was worked up and down at a fixed speed, so that while one side of the pillar was loaded the other was unloaded. By working this arrangement, tiltings were given to the column representing groups of waves with periods varying between twelve and twenty seconds. The resulting diagrams gave three results:—

(1) The amplitudes were greater than those due to steady loading.

The inference is that a horizontal pendulum does not correctly measure the amplitudes of the greater portion of a teleseismic disturbance, a conclusion long recognised by most seismologists. Why, therefore, it may be asked, are columns of figures relating to amplitudes continually appearing in earthquake registers? One reason is that they roughly give relative magnitudes for movements recorded at widely separated stations, and are, therefore, of great value in determining origins. When expressed in angular measure, if the corresponding periods of movement exceed two minutes it is likely that the pendulum has closely followed the tilting which has been recorded. Other reasons may be adduced to indicate that "amplitudes" have a value, and it is, therefore, desirable that they should be retained in registers.

(2) Although the period of the forced vibration was varied in different experiments, the resultant diagrams showed that the pendulum followed the tiltings of the pier.

The converse of this is found in the observation that pendulums with different periods will record the same periods for groups of waves in a given earthquake.

(3) The diagrams showed marked interference effects. For example, forced vibrations of fifteen seconds acting on a pendulum with a period of 16·5 seconds yield a series of throbbings very similar in appearance to many seen in a seismogram.

Although I entirely agree with Dr. Farr as to the existence of interference effects of this description, this by no means excludes the existence of reflection effects or echoes. To commence with, we will consider the first great echo, or Yuri Kaishi. The Yuri Kaishi, or return shaking, which frequently occurs about four minutes after the first shock, was recognised and christened long before the invention of modern seismographs. You feel it, you may see its effects, and, within a meizoseismic area, a seismograph is not required to give evidence of its existence. To regard it as an imaginary reinforcement of earth movement due to a want of synchronism in the period of the same and that of a horizontal pendulum is out of the question. The Yuri Kaishi rattles doors and windows, and causes people to leave their houses. My own view of the phenomena is that it is similar to what is seen when a bullet is dropped into the middle of a large tub of water. Waves travel outwards to the sides of the tub, where they are reflected, after which they converge at the centre from which they started. In nature, the reflecting surface may possibly be represented by the roots of mountain ranges. As these may be at varying distances from the origin of the disturbance, the reflections will give rise to complications at the focus. The transmitting medium I take to be that material beneath the heterogeneous superficial covering of our earth which transmits large waves with a constant velocity of about 3 km. per second.

A megaseismic primary and its echoes may be transmitted to very long distances, and as they travel may by reflection at other surfaces be still farther broken up into minor wave groups. Two wave groups within a megaseismic area might at a great distance from an origin be represented by four groups, and so on. This matter has not yet been carefully looked into, but evidence exists that wave groups increase in their number with distance from an origin (British Association Report, 1897, p. 68).

In support of the explanation I offer for the Yuri Kaishi it may be mentioned that the time interval between it and its primary is in those cases where we know the distance between an epifocal area and a supposed reflecting surface,

such as might be anticipated. As an illustration of this I will take the Californian earthquake of April 18. This originated along lines of great length on the western side of that country. The reflecting surface I take to be the Sierras, 200 miles distant. A wave group would travel to the Sierras and back in about four minutes, and this is approximately the time interval between the two first large wave groups in the few seismograms I have of that disturbance.

In offering these suggestions I concur with Dr. Farr that very frequently terminal vibrations of an earthquake present characteristics suggestive of interference effects, but it is premature to suppose that all the peculiarities of a seismogram are to be explained in the same manner. Rhythmical beats at an origin may result in rhythmical beats at a distance. JOHN MILNE.

Shide, Isle of Wight, September 5.

Remarkable Rainbow Phenomena.

ON Monday, September 3, a very heavy thunder-shower passed from west to east over the parish of Deerness, Orkney, from 5.30 p.m. to 6.25 p.m. When the dark nimbus cloud to the west had lifted its pall, the sun came out in great brilliancy. A rainbow now began to form to the north-east, but instead of the ordinary bow there was one of a bifurcated nature. Two stumps which coalesced on the horizon gradually developed into two magnificent bows, which met on both horizons, viz. north-east and south-west, but were about five or six degrees apart at the apex. All the colours of a radiant bow were present in both, and both had the colours arranged in the order of the primary bow. The secondary bow also appeared with the colours reversed and the same bifurcation, but in this case it extended only to about thirty or thirty-five degrees above the horizon, as secondaries generally do. As I had never seen or heard of anything like this, my first impulse was to find a cause. When the double rainbows were at their best, there was a bar of stratus cloud extending across the middle of the sun, and in breadth about one-sixth of its diameter. The two primary bows remained complete from 6.30 p.m. to 6.35 p.m., and without the arc of the apex for about another five minutes.

At first the bow to the south was the more complete, and finally the one to the north. However, after the sun had crept from behind the bar of cloud there were still double stumps clearly visible. If the cause here attributed be correct, then the only explanation of the bifurcated rainbow being visible after the cloud passed is, that from the points of the heavens where the rainbows were the bar might still be dividing the sun's rays. Nothing in my meteorological books indicated that this phenomenon had been previously seen. On inquiring as to what others had seen after the thunder-shower, two friends, one five miles and the other three miles almost directly west of me, saw only a perfect bow and its secondary. Others nearer the position I occupied saw what they called four rainbows, but had observed neither the coloration nor bifurcation clearly enough for descriptive purposes. M. SPENCE.

Deerness, Orkney.

The Mixed Transformation of Lagrange's Equations.

MR. A. B. BASSET, in a letter to NATURE of August 2, states that the theory of the mixed transformation was first given by himself in 1887, and refers to his treatise on "Hydrodynamics," vol. i., p. 171. If he will kindly look at my essay for the Adam's prize, pp. 61–4, he will find an elimination similar to that which he speaks of. The resulting modified functions appear to agree term for term. There is also the introduction of a "modified function" by which we can use Lagrange's equations for some of the coordinates and Hamilton's equations for the others. That essay dates from December, 1876, and was published in August, 1877. The method was afterwards explained without much change in all the editions of my "Rigid Dynamics" which follow that date, beginning with the fourth edition, 1882. E. J. ROUTH.

September 14.

THE RECENT CONTROVERSY ON RADIUM.

THE recent correspondence on the subject of radium, started in the *Times* by Lord Kelvin, has, after lasting nearly a month and causing widespread interest, apparently closed without any very definite conclusion being reached. Whatever opinion may be formed of the merits of the controversy, all must unite in admiration for the boldness with which Lord Kelvin initiated his campaign, and the intellectual keenness with which he conducted, almost single-handed, what appeared to many from the first almost a forlorn hope against the transmutational and evolutionary doctrines framed to account for the properties of radium. The weight of years and the almost unanimous opinion of his younger colleagues against him have not deterred him from leading a lost cause, if not to a victorious termination, at least to one from which no one will grudge him the honours of war. If peace and tranquility now result, and a measure of agreement is arrived at between conflicting views, it will be a result which all concerned will heartily welcome. The most ardent believer in the truth of the new doctrines cannot be other than satisfied that every feature and assumption that is admittedly speculative should be clearly recognised as such and separated from that which is not, if thereby the experimental foundations of the science of radio-activity are freed from further wordy and unprofitable controversy. There seems now to be a reasonable prospect that this has been secured.

Lord Kelvin's opening challenge (August 9) was broad and sweeping. He took exception to the statement, made by the writer in opening the discussion on the evolution of the elements at the British Association at York, that the production of helium from radium has established the fact of the gradual evolution of one element into others, and denied that this discovery affected the atomic doctrine any more than the original discovery of helium in cleveite. The obvious conclusion was that both cleveite and radium contained helium. He also stated that there was no experimental foundation for the hypothesis that the heat of the sun was due to radium, and ascribed it to gravitation.

The challenge was taken up on the other side successively by Sir Oliver Lodge, the Hon. Mr. Strutt, and other well-known authorities, and it soon became apparent that for argument at least Lord Kelvin on his side had to rely practically on himself alone. Prof. Armstrong, it is true, immediately enrolled under Lord Kelvin's banner, and entered the lists with an embracing criticism of physicists in general, whom, he declared, are strangely innocent workers under the all-potent influence of formula and fashion. He made the statement that no one had handled radium in such quantity or in such manner that we can say precisely what it is, and throughout put the word *radium* in inverted commas.

Whether or no his opponents are all as innocent and ignorant as Prof. Armstrong imagines, the fact remains that, except for this *ex cathedra* utterance and a leading article, argument against the accepted view there was little or none except that contributed by Lord Kelvin himself. Prof. Armstrong's letter merely served to provide Sir Oliver Lodge with justification for his favourite theme, which appears to be that whereas chemists have an instinct of their own for arriving at their results, reason is the monopoly of the physicist, whose results the chemist usually manages to absorb in the end. No better argument against the unfairness of this could be provided than by the history of radio-activity itself, which